Apart from the Somali giraffe, which certainly differs in colour and marking more from all the others than do the latter *inter se*, the aforesaid tendency to a gradation (with many local side-developments) from the northern three-horned and white-stockinged to the southern two-horned and spot-legged type points to the advisability of regarding the local colour-forms as races rather than species (in the modern sense of both terms). It should be mentioned, however, that at present, at all events, there is no sign of one local form grading into another, although subsequent discoveries may prove this to exist.

Then comes the question, are these local forms constant and invariable (save for individual tendencies towards albinism or melanism)? Dealing solely with available facts, and not admitting hypothesis, the answer to this, so far as our present information goes, is in the affirmative. Of course, additional specimens of each form are desirable, but all the examples of each type at present available point to the conclusion that such types are constant locally, and it is therefore obvious that it is incumbent on those who dispute this assertion to substantiate their objections by producing specimens showing individual variation in one and the same locality.

As regards the evidence for constancy of local type, it may be mentioned that the herd of Nubian giraffes formerly in the Zoological Society's Gardens, together with the numerous specimens that have of late years been imported into Europe, are amply sufficient to demonstrate the absence of individual variation in this case. The proof of constancy of type is nearly equally strong in the case of the Cape giraffe, despite the fact that as we proceed north a change in the nature of the markings is noticeable. The Baringo and Kilimanjaro giraffes, allowing for marked differences according to age and sex, are also known by a considerable number of specimens, so that there is every probability that their respective types of coloration are fairly constant, and the evidence for such constancy is still more satisfactory in the case of the Somali giraffe. As regards the other named forms, it must be admitted that their right to separation rests on the evidence of single specimens. Still, if constancy of type occurs in the other forms, the presumption is that it also obtains in these.

If, of course, Kilimanjaro giraffes were met with among a Baringo herd, or *vice versâ*, the case for the racial distinctness of the local forms would be at once demolished, but no evidence of such an admixture of type has ever been recorded. Until this is observed, we are accordingly entitled —or rather compelled—to regard the differences in the colour and markings of giraffes from different localities as indicating local races precisely analogous to those of the bonte-quagga, or Burchell's zebra. The extent of area covered by these local forms, whether some of them may be anything more than individual variation, and whether on the borders of their respective ranges they interbreed with the neighbouring races, or, as is more probable, keep perfectly distinct, are factors in the question still awaiting definite answers.

In conclusion, reference may be made to the extremely important and valuable additions to our knowledge of these animals which have resulted from the specimens collected by Major Powell-Cotton during his recent expedition to East Central Africa. The only pity is that, by reason of the game-preservation laws, he was prevented from bringing away such a series of examples of the different local forms as would have sufficed to convince even the most sceptical of their respective constancy to a common type.

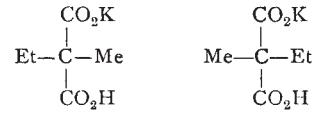
R. L.

A DIRECTED SYNTHESIS OF AN ASYMMETRIC COMPOUND.

ALL previous attempts to synthesise an optically active carbon compound have been based on one principle; they have involved the combination of an inactive compound, containing an ethylene linkage or a carbonyl group, with an optically active substance to form an ester or glucoside, and the subsequent treatment of this product in such a way as to attach two different groups to a previously ethenoid carbon atom so as to render it asymmetrical. As Prof. Japp and others have pointed out, the two possible isomerides should not necessarily be formed in equal quantities, so that on hydrolysing the new ester or glucoside one of the isomerides would preponderate, and therefore an optically active product should be obtained. Prof. Kipping reduced the bornyl ester of benzoyl formic acid to the bornyl ester of mandelic acid, hoping to obtain an optically active mandelic acid. Prof. Fischer and M. D. Slimmer added hydrogen cyanide to helicin; they then hydrolysed the nitrile and subsequently the glucoside, with the object of preparing active oxymandelic acid.

Prof. Kipping's product proved to be inactive, and it was not conclusively established that that obtained by Fischer was active.

In the last number of the *Berichte*, Prof. Marckwald, of Berlin, describes the synthesis of active *l*-valerianic acid. Methylethylmalonic acid forms two acid salts, which in the case of the potassium salt are enantiomorphously related



and will possess the same solubility, whereas the two acid salts with an optically active base, such as brucine, will in general have different solubilities. Methylethylmalonic acid loses carbon dioxide when heated, forming methylethylacetic acid, which contains an asymmetric carbon atom; it is to be supposed that from the acid brucine salts of the malonic acid the free carboxyl group will be preferentially eliminated. Led by these considerations, Prof. Marckwald crystallised the less soluble brucine salt from the mixture of the two in the expectation of obtaining a material in which presumably the one form of the asymmetric compound would preponderate. This salt was heated at 170°. The valerianic acid obtained was optically active to the extent of $-1°.7$ per 10 c.c., which may be taken as an indication of the presence of 10 per cent. of *l*-valerianic acid.

BAROMETRIC SEESAWS.

PROF. HOFRATH JULIUS HANN has recently contributed an important paper to the Vienna Academy, an abstract of which is printed in the *Akademischen Anzeiger*, No. 1, of the Kaiserliche Akademie der Wissenschaften in Wien. This paper is entitled "Die Anomalien der Witterung auf Island in dem Leitraume 1851 bis 1900 und deren Beziehungen zu den gleichzeitigen Witterungsanomalien in Nordwest Europa." In this Prof. Hann discusses the relationship between the monthly and yearly means of the temperature, pressure and rainfall of Stykkisholm, in Iceland, for the longest time available, the temperature variations at Greenwich, Brussels and Vienna, the pressure and rainfall variations at Brussels and the pressure variations at Vienna.

The results at which he has arrived are of very great interest, for they show that there is a most intimate connection between Icelandic meteorology and that of north-west Europe. For a full account of these the reader must refer to the abstract itself, but some of the results may be briefly summarised here. In the first place, for the three winter months the pressure variations of north-west and middle Europe are for the most part simultaneously of opposite sign to those at Stykkisholm, while the same reversal occurs to a slight extent with the temperature and rainfall. Again, when the pressure variation for a month in Stykkisholm is negative, the probability for a positive temperature variation in north-east and middle Europe is 0·82, and *vice versâ* with a probability of 0·73. Again, contrasting temperature and pressure variations, the following results were obtained:—

	No. of cases	Mean variation: Pressure, Stykkisholm	Mean variation: Temp. Greenwich Brussels	Probability of sign of temp. variation
		mm.	°C.	
Winter half year ...	67	+8·6	−1·5	0·81
Summer half year...	55	+3·8	−0·5	0·65
Winter half year ...	72	−7·7	+1·4	0·90
Summer half year...	50	−5·0	+0·7	0·76

Prof. Hann shows further that the probabilities of positive temperature variations at Greenwich and negative pressure variations at Stykkisholm, and *vice versâ*, are 0·83 and 0·85 respectively. In the case of the Azores he shows that a similar reversal with Stykkisholm occurs. Interesting results are also obtained when he considers the new station at Angmagsalik, in Greenland.

THE AMERICAN ASSOCIATION.

THE annual meeting of the American Association for the Advancement of Science was held at St. Louis on December 26, 1903, to January 1. The address delivered by the president of the association, Prof. Ira Remsen, appeared in NATURE of January 28; and extracts from the addresses of presidents of some of the sections are given below.

ATOMS AND ELEMENTS.[1]

Is matter continuous or discrete? argued the opposed schools of Grecian philosophy led by Leucippus, Democritus and Epicurus, and dominated by Aristotle. Despite the clarity of the statements of the Roman Lucretius,[2] the atomic hypothesis received scant attention until the seventeenth century of the Christian era, when Galileo's experimental science assailed Aristotelian metaphysics and demanded verification of the premises of that philosophy which had governed all the schools of Europe for two thousand years.[3] While Gassendi, Boyle, Descartes, Newton, perhaps Boscovich, Lavoisier, Swedeborg, Richter, Fischer and Higgins had to do with our modern atomic theory, Dalton one hundred years ago "created a working tool of extraordinary power and usefulness" in the laws of definite and multiple proportions. As Clarke[4] remarked, "Between the atom of Lucretius and the Daltonian atom the kinship is very remote." Although the lineage is direct, the work of Berzelius, Gmelin and others; the laws of Faraday, Gay Lussac, Avogadro, Dulong and Petit; the reformations of Laurent and Gerhardt, but particularly Cannizzaro; the systematisations of de Chancourtois, Newlands, Hinrichs, Mendeléeff and Lothar Meyer; the stereochemistry of van 't Hoff and Le Bel have imperialised the ideas of the Manchester philosopher, so that the conceptions of the conservative atomists of to-day are quite different from those at the beginning of the closed century.[5]

The Daltonian ideas had scarcely reached adolescence before Prout (1815), giving heed to the figures concerned, would have all the elements compounded of hydrogen. The classical atomic mass values obtained by sympathetic Stas and the numerous investigations of those who followed him, with all the refinements human ingenuity has been able to devise, temporarily silenced such speculations, but not until Marignac had halved the unit, Dumas had quartered it, and Zängerle, as late as 1882, insisted upon the one thousandth hydrogen atom.

The notion, like Banquo's ghost, will ever up, for if one may judge from the probability calculations of Mallet (*Phil. Trans.*, clxxi., 1003, 1881) and Strutt (*Phil. Mag.*, (6), i., 311), a profound truth underlies the now crude hypothesis.

Crookes (*Chem. News*, lv., 83, 1886), from observations made during prolonged and painstaking fractionations of certain of the rare earths, supported his previously announced "provisional hypothesis" as to the genesis of the elements from a hypothetical *protyle*, which existed when the universe was without form and void. He designated those intermediate entities, like yttrium, gadolinium and didymium, "meta-elements,"[1] a species of compound radicles, as it were. *Urstoff*, fire mist, protyle, the ultra-gaseous form, the fourth state of matter (Crookes, Royal Societies, June 10, 1880) was condensed by a process analogous to cooling; in short, the elements were created. The rate of the cooling and irregular condensation produced "the atavism of the elements," and this caused the formation of the natural families of the periodic system. Marignac (*Archives des Sciences Physiques et Naturelles*, 17–5; *Chemical News*, lvi., 39), criticising this hypothesis, states:—"I have always admitted[2] the impossibility of accounting for the curious relations which are manifested between the atomic weights of the elements, except by the hypothesis of a general method of formation according to definite though unknown laws; even when these relations have the character of general and absolute laws."

Further, "I do not the less acknowledge that the effect of constant association of these elements is one of the strongest proofs that can be found of the community of their origin. Besides, it is not an isolated fact we can find other examples such as the habitual association in minerals of tantalum, niobium, and titanium."

The peculiar discharge from the negative electrodes of a vacuum tube was investigated many years ago by Hittorf and Crookes, who arrived at the conclusion that it was composed of streams of charged particles. All are familiar with the very recent proposed "electrons" and "corpuscles" resulting from the beautiful physical researches of Lodge and J. J. Thomson. These appear to have caused a trembling in the belief of many in the immutability of the atom, and the complete abandonment of the atom is seriously discussed by others.

Although by chemical means, so far, we have been unable to break up the atoms, apparently electrical energy, in the form of kathode rays, for example, follows the grain of atomic structure. Some advanced thinkers look upon the atoms as disembodied charges of electricity. Ostwald has taught it. Electric charges are known only as united to matter, yet Johnstone Stoney and Larmor, have speculated on the properties of such charges isolated. "Such a charge is inertia, even though attached to no matter, and the increase of inertia of a body due to electrification has been calculated by both Thomson and Oliver Heaviside, the conception accordingly being advanced that all inertia is electrical, and that matter, as we know it, is built up of interlocked positive and negative electrons. If it were possible in any mass of matter to separate these electrons then matter would disappear and there would remain merely two enormous charges of electricity." We are aware of phenomena attributed to the negative electrons; we await anxiously the announcement of the positive electrons.

We do know, however, as A. A. Noyes says, that "there exists in the universe some thing or things other than matter which, by association with it, give rise to the changes in properties which bodies exhibit, and give them power of producing changes in the properties of other bodies."

Shall we say, as does Remsen, "An element is a substance made up of atoms of the same kind?" Can we say that it is not? Venable (the "Definition of the Element," *Am. Chemist*, 1875, 23) truly says: "An element is best defined by means of its properties." These conceits are not exclusive. The properties are the result of the action of physical forces and chemical affinity, whatever that may be. Certain of the novel atmospheric gases have so far responded but poorly to the latter, as predicted before their discovery by Flawitzsky, Julius Thomsen and de Boisbaudran in 1887.

The following simpler definition has finally served as my guide: *An element is that which has not been decomposed, so far as we are aware, into anything other than itself.* In short, it is consistent.

We have decided to define an element by its properties. The alterations produced in the properties of the most characteristic elements by the presence of small amounts of foreign substances are evident in steel. The influence of arsenic upon the conductivity of copper is well known, and Le Bon (*Compt. rend.*, cxxxi., 706, 1900) has recently shown

[1] Abridged from an address delivered before the Section of Chemistry of the American Association by Prof. C. Baskerville.

[2] "Nature reserving these as seeds of things
Permits in them no minish nor decay;
They can't be fewer and they can't be less."
Again, of compounds—
"Decay of some leaves others ree to grow
And thus the sum of things rests unimpaired."
Book ii., 79.

[3] See "The Atomic Theory," the Wilde Lecture by F. W. Clarke at Dalton Celebration, May, 1903.

[4] *Loc. cit.*

[5] While I have examined much of the original literature, Venable's "History of the Periodic Law" has been most helpful. I have, furthermore, had the privilege of reading very carefully the manuscript of a work entitled "The Study of the Atom" (in press), by Dr. Venable.

[1] Address before Chemical Section of the British Association, *Chem. News*, liv., 117, 1885.

[2] Remarks made in 1860–5 after publication of Stas's "Researches on Atomic Weights," *Archives*, ix., 102, 24–376.